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THE APPLICATION OF DIFFERENTIAL
SCORING METHODS TO PK TESTS

BY A. M. J. MITCHELL AND G. W. FISK

1. *Introduction*

In a previous paper¹ we described the application of a differential scoring technique to an ESP test using Clock cards. Each card bore a picture of a clock-face with an hour-hand pointing exactly to one of the hours I-XII and it was the object of the percipient to 'guess' the hour to which the hand was pointing. The results indicated that the percipients tended to record 'guesses' both on the target hour and on hours near to it, whilst they avoided hours further away, i.e. the Angular Error of a 'guess' tended towards zero. By using a differential scoring technique which, to some extent, took into account this effect, it was found that higher *t* (CR) values were obtained than those derived using the usual 'direct hits only' method. In view of the advantages of the differential scoring technique, e.g. apparently increased sensitivity in the detection of ESP, it seemed worthwhile to consider its application to PK tests and this paper describes some preliminary experiments using dice.

2. *Basis of differential scoring*

In the usual or 'bull's-eye' scoring method (B) the outcome of a single trial may fall into one of two categories: a hit or a miss. These are scored 0 or 1 respectively. The differential scoring method (D) divides the outcome into three or more categories (by differentiating between various types of miss), and each category is given a different score. Thus the B scoring method is a special case of the more general D method and this is reflected in the statistics of the score distributions.

¹ ESP experiments with Clock Cards: a new technique with differential scoring, *Jnl. S.P.R.*, Vol. 37, No. 673, January-February 1953.

The division into categories, and the scores to be given, are decided arbitrarily and form a hypothesis to be tested by application to experimental data. Two of the many possible differential scoring methods are proposed below and applied, together with the usual B system, to PK tests with dice. The first is termed the Die Orientation (DO) method and the second the Face Value (FV) method.

3. *Die Orientation method*

When a die has been thrown for a target, the face matching the target will be found in one out of six possible positions: horizontal on top (a hit), vertical (four possible positions), or horizontal and in contact with the surface upon which the die has been thrown. This suggests the hypothesis (by analogy with Clock cards) that the 'error' of a throw is the angular rotation required to bring the matching face to the top horizontal position. Thus we have three categories, horizontal on top, vertical, and horizontal at the bottom, to which may be applied the scores of 0, 1, and 2 right-angles respectively. The following example illustrates how the six possible results of a single throw could be scored in practice, where it is more convenient to read the upper face of a die than to search for the matching face:

TARGET 6

Upper die face	-	1	2	3	4	5	6
Matching face	-	Bottom	Side	Side	Side	Side	Top
Score (error)	-	2	1	1	1	1	0

It will be noted that the smaller the score, the more accurate is the throwing—in contrast to the usual B method where the reverse is the case.

An analysis of the distribution of total score for the DO scoring method is given in Appendix I. The distribution is shown to be adequately normal with $MCE = n$ and $SD = \sqrt{n/3}$. The potential sensitivity of the DO method over the B method is calculated and illustrated in Fig. 1 of that Appendix. In the tests described below, a pair of dice was cast to make a certain number of trials (usually 100) on each target and the number of times each of the faces 1 to 6 occurred was recorded. The total number of hits and the deviation from MCE was calculated in the usual way for the B scoring method. In Appendix II it is shown that the deviation from MCE for the DO method is simply the difference between

the number of times the target face occurred and the number of times the complementary¹ face occurred.

The tests described in this section were devised and arranged by G. W. F.; they started in February 1951 and results are given up to December 1952. Some subjects are continuing with the tests. The basic experiment comprised the throwing of a number of dice for a succession of targets, each displayed for 24 hours and changed at approximately 8 a.m. daily by G. W. F. at Long Ditton, Surrey. The subjects, ten in all, were situated some miles away from the targets—which were unknown to them. Thus no subject, at any time, was able to appraise the results of his trials. Significant scores, subject to the usual provisos, are presumed to be due to a combination of ESP and PK. The dice ($\frac{5}{8}$ in. cube) were provided by G. W. F. after testing for any appreciable bias and were cup-thrown, subjects sending records of the throws to G. W. F. at intervals. In general, the throwing was not witnessed. G. W. F. selected the targets initially by throwing a die, but over the greater part of the test period he used a table of random numbers. Occasionally, due to the absence of G. W. F., it was not possible to change the target for several days, and over the latter period of the tests, several subjects were using the same targets. One variant of the above procedure was used for subject J. B. (Dr J. Blundun). Having successfully completed one test, this subject agreed to undertake another in which the targets were selected by means of a Latin Square. G. W. F. supplied J. B. with six sealed envelopes labelled A to F, each containing a target card bearing 1, 2, 3, 4, 5, or 6 spots to act as targets. The envelopes were filled in the dark so that G. W. F. did not know which envelope contained which target. The order in which J. B. took the targets was determined by a 6×6 alphabetical Latin Square supplied by G. W. F. At the end of 3,600 trials the unopened sealed target envelopes were sent to Dr D. J. West who identified and informed G. W. F. of the targets. The latter was then able to assess the scores. All the results of all subjects have been independently checked by A. M. J. M.

Before giving a summary of results, the scores for the first subject (S. M.) are shown below in a modified Chi-Square table (Table 1) which was found convenient for collating data. The significance of the results will be calculated in detail to illustrate the general procedure.

¹ The complementary face is the face opposite the target face, i.e. target 6 has complementary face 1; target 5, complementary face 2; target 4, complementary face 3, and so on. All dice used were found to be manufactured so that the sum of spots on opposite faces totalled 7.

TABLE I
MODIFIED CHI-SQUARE TABLE FOR ESP/PK RESULTS
SUBJECT S.M. Throws/day = 200 No. of days = 3 $n = 600$

		TARGET											Row Total
		1	2	3	6	5	4	1	2	3	6	5	
A	1	41		46			29						116
C	2			33			41	36					110
T	3			35			26	27					88
U	6						35	27		35			97
A	5						38	27		30			95
L	4						31	42		21			94
Diagonal Total							107	113	108	74	98	100	600
Target Frequency							1	1	0	1	0	0	3

The numbers within the 'staircase' refer to the number of times each die face occurred for a particular target face. For example, when the target face was 1, there were 41 ones, 36 twos, 27 threes, 27 sixes, 27 fives, and 42 fours. The total number of direct hits or bull's-eyes is found by summing down the diagonal running from the square 'Target 1—Actual 1,' to square 'Target 4—Actual 4.' The result is seen to be 107 hits or 107 occasions with score 0. The total number of complementary faces—for each of which the score is 2—is found from the diagonal running from Target 6—Actual 1 to Target 3—Actual 4 (Result, 74). The four other diagonal totals correspond to score 1. The row totals give the occurrence of each face during the throwing. The target frequency row gives the number of times each face was used as target: e.g. the targets 4, 1, and 3 were used once each while targets 2, 6, and 5 were not used.

The calculations then proceed as follows :

B SCORING METHOD

$$MCE = np = 600 \times 1/6 = 100 \quad \text{Dev. from MCE} = 107 - 100 = +7$$

$$SD = \sqrt{(npq)} = 600 \times 1/6 \times 5/6 = \sqrt{83.3} = 9.11$$

$$\text{Hence } t = 7/9.11 = 0.77 \quad P = 0.44 \quad \text{Odds } 1.3 : 1 \text{ against}$$

DO SCORING METHOD

$$\begin{aligned} \text{MCE} = n = 600 \quad \text{Dev. from MCE} &= 600 - (113 + 108 + 98 + 100 + 2 \\ &\quad \times 74) \\ &= 600 - 567 = +33 \end{aligned}$$

(or more rapidly, $107 - 74 = +33$)

$$\text{SD} = \sqrt{(n/3)} = \sqrt{(600/3)} = \sqrt{200} = 14.14$$

$$\text{Hence } t = 33/14.14 = 2.33 \quad P = 0.020 \quad \text{Odds } 49 : 1 \text{ against}$$

These results suggest that ESP/PK is operating to minimise the orientational error of the die. The DO scoring method takes more account of this effect than the B method, by an amount which is reflected in the difference between the two t values. The result is of particular interest since S. M. obtained successful results on Clock card tests, showing a higher t value under the differential scoring method than with the usual 'direct hits' method.

The results for the ten subjects are summarised in Table 2.¹ Modified Chi-Square tables for each subject—except S. M.—are given in Appendix III.

4. *Discussion of results using DO scoring method*

In six out of eleven experiments the DO method gives higher t values than the usual B method. The group result shows the same effect which is associated with an excess occurrence of score 0 and a deficit of score 2. Five subjects achieved a t value of 2.00 or over by the B method—and four by the DO method—although only one subject produced a score of comparatively high significance. The group result is significant, with odds of 4,000 : 1 against, for the B method, compared with 50,000 : 1 for the differential DO method. These results support the hypothesis that, on occasion, ESP/PK operates so as to orientate the die to the desired position and avoids turning the die so that the target face is in contact with the throwing surface.

A weakness in the experiments (except that of J. B. (2) which does not support the hypothesis) is that targets were not exactly equalised. It is hoped that a means will be found to correct the results for this effect and also for dice bias. However, it seems probable that although the absolute t values are subject to correction, the differences between the B and DO t values are not likely to be seriously affected.

¹ Since this report was sent to press, Dr J. Blundun has completed a third test of 5,000 trials which was conducted in the same way as her first. The scores were again above MCE, the t value calculated by the DO method being 1.98, compared with 1.54 by the B method. The combined results for all three experiments give corresponding t values of 4.51 and 4.15 respectively—odds of 148,000 : 1 against compared with 30,000 : 1 against.

TABLE 2
SUMMARY OF RESULTS FOR 10 SUBJECTS ON ESP/PK TESTS

Subject	Location	Target dist. miles	Total throws <i>n</i>	Freq. of occurrence (Dev. from MCE) for scores	Scoring Method			
				0	1	2	B	DO
S.M.	Birmingham	110	600	7	+19	-26	+0.77	+2.33
P.V.	France	160	646	+20.3	-1.6	-18.6	+2.14	+2.66
C.V.	"	"	646	+9.3	-21.6	+12.3	+0.98	-0.20
J.B.(1)	Devonshire	170	10,000	+117.3	-13.6	-103.6	+3.15	+3.83
A.N.	London	15	2,700	-14	+10	+4	-0.72	-0.60
A.I.B.	Devonshire	170	900	+2	+15	-17	+0.18	+0.271
B.R.B.	"	"	900	-27	+24	+3	-2.41	-1.73
J.A.C.	Kingston	3	10,000	+0.3	+30.3	-30.6	+0.01	+0.54
J.B.(2)*	Devonshire	170	3,600	+53	-52	-1	+2.32	+1.56
P.A.C.	Liverpool	200	5,000	+49.6	-16.3	-33.3	+1.96	+2.03
J.W.	Eire	320	5,000	+54.6	-41.3	-13.3	+2.07	+1.60
Total			39,992	+272.6	-48.3	-224.3	+3.66	+4.31
				Chi-Square (2 d.f.) =18.79 Odds greater than 10,000 : 1 against			Odds 4,000 : 1 against	
							Odds 50,000 : 1 against	

* Latin Square targets

5. *Face Value method*

This method was developed for PK tests where dice are thrown alternately for 'high' and 'low' targets. In experiments devised and arranged by A. M. J. M., eight subjects were asked to make six throws of six dice per throw, for the maximum possible score. They were told that most weight would be given to sixes and successively less weight to the remaining faces in descending order. The procedure was then repeated, aiming for low faces, most weight being given to ones etc. This total of 72 throws comprised a test session and subjects continued, not more frequently than once a day, until 24 sessions had been completed. Subjects carried out the tests in their own homes but all tests were witnessed by competent observers. The dice were cup-thrown and all score sheets were returned to A. M. J. M. for checking.

The method of scoring is illustrated in the following table :

TABLE 3
SCORING FOR 'HIGH AND LOW DICE' TEST

Actual face	High target						Low target					
	1	2	3	4	5	6	1	2	3	4	5	6
Score	5	4	3	2	1	0	0	1	2	3	4	5

It will be noticed that it is the 'error' of the throws which is being measured. It can be shown, by using the method of generating functions¹ as in Appendix I, that the distribution of total error is adequately normal with $MCE=5n/2$ and $SD=\sqrt{(35n/12)}$. This particular test can also be appraised using the B method, by noting only those occasions when the six face occurred during throws for high scores, and the one face when throwing for low scores. Further, the DO method can be applied, the relevant faces being those with score 0 and 5 as defined by the FV method. Accordingly the results have been appraised by all three methods and a summary is given in Table 4.

Although only one score with a t value exceeding 2.00 is found, the FV method gives higher t values than the B method for three subjects (O. C., A. M., and N. W.). The DO method gives a higher t value than the B method for only one subject (N. W.). However, these results are considered to be inconclusive from the point of view of assessing the value of these two differential scoring methods.

¹ The generating function in this case, is :

$$g.f. = (p_0u^0 + p_1u^1 + p_2u^2 + p_3u^3 + p_4u^4 + p_5u^5)^n$$

where p_0, p_1, \dots, p_5 , are the probabilities of score 0, 1 ... 5
e. 1/6. n is the number of trials.

TABLE 4
SUMMARY OF RESULTS FOR 'HIGH AND LOW DICE' TESTS
 $n = 72 \times 24 = 1728/\text{subject}$

Subject	Freq. of occurrence (dev. from MCE = 288) for score						Total score	<i>t</i> value for scoring method		
	0	1	2	3	4	5		B	DO	FV
L.J.	+13	-5	-14	-13	+13	+6	+10	+0.84	+0.29	-0.14
O.C.	+16	+21	-7	-27	-6	+3	-83	+1.03	+0.54	+1.17
D.L.	-24	-4	+33	-8	-2	+5	+55	-1.55	-1.21	-0.78
A.M.	+10	+19	+2	-23	-15	+7	-71	+0.65	+0.13	+1.00
E.M.	+9	+2	-40	+4	+20	+5	+39	+0.58	+0.17	-0.55
S.W.	+18	-5	-26	-3	+17	-1	-3	+1.16	+0.79	+0.04
N.W.	-18	+11	-25	+3	-3	+32	+118	-1.16	-2.08	-1.66
L.S.	-18	+18	+6	+6	-12	0	0	-1.16	-0.75	0
Total	+6	+57	-71	-61	+12	+57	+65	+0.39	-0.84	-0.32

6. Discussion and conclusions

Two differential scoring methods have been proposed and tested against PK data. The results, using the first method, suggest that ESP/PK is not an 'all-or-nothing' phenomenon but may sometimes act without producing a direct hit. A tendency is noted for the target face on the thrown die to avoid coming to rest in contact with the throwing surface. If a direct hit does not occur, the target face is more likely to be found in a vertical position. The differential scoring method takes more account of this effect than the usual 'direct hits' method and gives higher *t* values than the latter. It should be noted that the theoretical MCE and SD have been used in evaluating the data and we have not been able to correct the results for the effects of target inequality and dice bias. While the absolute *t* values may require some correction, it is not expected that the *difference* between the 'direct hits' and differential *t* values will be much affected. As the differential scoring method can be applied to the usual PK test data without much labour, it is suggested that it might be worthwhile re-examining some of the existing PK data for evidence of the Die Orientation effect.

The second differential scoring method provides a way of estimating the significance of the total score when equal numbers of dice are thrown, first for high faces and then for low. Allowance is made, on a sliding scale, for faces other than the six when throwing for low faces. When applied to test data it was found that the method gave no conclusive advantage over the usual 'direct hits' method. Here again it might be worth trying the method on appropriate existing data.

As with Clock cards, it would be valuable to be able to determine the significance of *differences* between t values computed by differential and 'direct hit' methods for the same data. This statistical problem has not yet been solved.

In conclusion, it is stressed that only two of the many possible differential scoring methods have been applied to a limited collection of data. It may well be that existing data contain useful clues to the nature of PK (and ESP) which might be brought to light by the use of more refined appraisal methods of the differential type.

7. Acknowledgment

It is a pleasure to acknowledge the co-operation of Dr D. J. West in this work and also the patience and recording accuracy of the subjects who took part in the experiments.

APPENDIX I

DISTRIBUTION OF TOTAL SCORE IN THE DIE ORIENTATION PK TEST

In the Die Orientation scoring method, the problem is to determine the significance of the deviation of an observed score from the mean. The first step is to determine the nature of the score distribution, i.e. the probability of occurrence of any given score. The method of generating functions¹ will be used to derive this information because the MCE and SD of the distribution can readily be obtained and comparison made with the Normal distribution. It will be shown that the desired distribution is effectively Normal, so that the standard tables of the probability function may be used for estimating significance.

The generating function is given by,

$$g.f. = [p_0u^0 + p_1u^1 + p_2u^2]^n$$

Here, p_0 , p_1 and p_2 are the probabilities of obtaining score 0, 1 and 2 respectively in a single call. The probability of obtaining a score S , in n trials, will be the coefficient of u^S in the expansion of the g.f.

The factorial moment generating function is obtained by re-writing the g.f. with u replaced by $(1 + \alpha)$. Then

$$f.m.g.f. = [(p_0 + p_1 + p_2) + \alpha(p_1 + 2p_2) + \alpha^2p_2]^n$$

Substituting values for p_0 , p_1 and p_2 —1/6, 4/6 and 1/6—

$$f.m.g.f. = [1 + \alpha + \alpha^2/6]^n$$

The MCE is the coefficient of α in the expansion of the f.m.g.f.

$$i.e. MCE = n$$

¹ See, for example A. C. Aitken, *Statistical Mathematics*, Edinburgh, Oliver & Boyd, (1945).

It is now necessary to set up the f.m.g.f. about the MCE viz. f.m.g.f. about mean $= (1 + \alpha)^{-n} (1 + \alpha + \alpha^2/6)^n$

On expanding this expression, we find,

$$\text{f.m.g.f. about mean} = 1 + \frac{n}{3} \cdot \frac{\alpha^2}{2!} - n \cdot \frac{\alpha^3}{3!} + \frac{\alpha^4}{4!} \cdot \frac{n}{3} (11 + n) + \text{etc}$$

The coefficients of $\alpha^2/2!$, $\alpha^3/3!$, $\alpha^4/4!$ are designated $\mu_{(2)}$, $\mu_{(3)}$ and $\mu_{(4)}$. From these, the moments μ_2 , μ_3 and μ_4 can be found, the SD computed and comparison made with the Normal distribution.

$$\mu_{(2)} = n/3 \quad \text{Now } \mu_2 = \mu_{(2)} = n/3 \text{ i.e. } SD = \sqrt{(n/3)}$$

$$\mu_{(3)} = -n \quad \mu_3 = \mu_{(3)} + 3\mu_{(2)} = -n + n = 0$$

$$\text{Coefficient of skewness, } \beta_1 = \mu_3^2 / \mu_2^3 = 0$$

$$\mu_{(4)} = n(11 + n)/3 \quad \mu_4 = \mu_{(4)} + 6\mu_{(3)} + 7\mu_{(2)}^2 = n(11 \times n)/3 - 6n + 7n/3 = n^2/3$$

$$\text{Coefficient of flattening, } \beta_2 = \mu_4 / \mu_2^2 = n^2/3 \times 9/n^2 = 3$$

The Normal distribution is a bell-shaped symmetrical curve, so that the coefficient of skewness is zero. Also the coefficient of flattening is 3. Hence, the distribution of total score in the Die Orientation method is closely Normal, with MCE = n and $SD = \sqrt{(n/3)}$.

The corresponding analysis for the 'direct hits' scoring method, starts from the generating function

$$g.f. = [pu^1 + qu^0]^n \quad (\text{notice the close similarity with the DO generating function})$$

Here, p and q are the probabilities of a hit and a miss, respectively, i.e. $1/6$ and $5/6$. It turns out that the distribution of total score is adequately Normal, provided n is large, with MCE = $n/6$ and $SD = (5n/36)$.

It is now of interest to compare the t values derived by the 'direct hits' and DO scoring methods for the same data. We consider a general case where H direct hits and a DO score of S have been obtained in n trials.

Direct hits t value

$$\text{Now, } t = \frac{\text{deviation}}{SD} = \frac{H - n/6}{\sqrt{(5n/36)}} = \frac{6H - n}{\sqrt{5n}}$$

It is convenient to express the number of direct hits as a fraction k , of the total possible, i.e. n . Hence,

$$H = kn$$

$$\text{Then } \frac{t}{\sqrt{n}} = \frac{6k - 1}{\sqrt{5}}$$

DO t value

$$t = \frac{\text{deviation}}{SD} = \frac{n - S}{\sqrt{(n/3)}}$$

$$\text{or } \frac{t}{\sqrt{n}} = \sqrt{3}(1 - S/n)$$

Now, we can regard H as fixed and investigate the possible range of S , which of course is limited by H . Noting that the score for a direct hit is zero, there are $(n-H)$ trials for which the individual score is not zero so that the minimum possible score is $(n-H)$ in these circumstances i.e. all the trials which are not direct hits have score 1. Similarly the maximum possible score will be $2(n-H)$. The t value can then be written as :

$$\frac{t}{\sqrt{n}} = \sqrt{3}[1 - (n-H)(1 \text{ or } 2)/n]$$

$$\text{or, } \frac{t}{\sqrt{n}} = \sqrt{3}[1 - (1-k)(1 \text{ or } 2)]$$

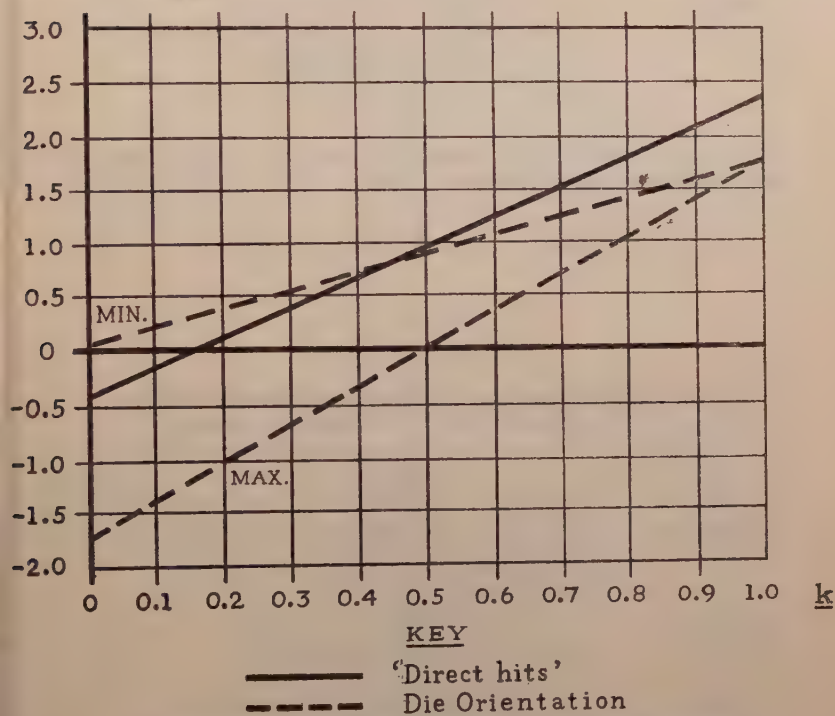


FIG. 1: Comparison of t values for 'direct hits' and Die Orientation scoring methods.

The t values for the 'direct hits' and DO scoring methods are compared in Fig. 1. It will be noted that when k is small, the usual state of affairs in ESP and PK tests, the DO t value can lie within a wide range of values astride the 'direct hits' t value. Thus the DO scoring method is potentially more sensitive over the range of practical interest.

Further work is now required to determine the significance of the difference between the 'direct hits' and DO t values, when they are derived from the same test data.

APPENDIX II

RAPID METHOD FOR OBTAINING DEVIATION FROM MCE IN THE DO SCORING METHOD

Let n be the total number of throws for one target and A, B, C, D, E, F be the number of times the one, two, three, four, five and six faces occur in the throwing.

Then
$$n = A + B + C + D + E + F$$

If the target face is six then the complementary face is one and the score is,

$$S = 2 \times A + 1 \times (B + C + D + E) + 0 \times F$$

$$= 2A + B + C + D + E$$

$$\text{Dev. from MCE} = n - (2A + B + C + D + E)$$

$$= F - A$$

i.e. the Dev. from MCE is the total number of direct hits, less the total number of complementary faces.

APPENDIX III

MODIFIED CHI-SQUARE TABLES FOR ESP/PK RESULTS SUBJECT P.V. Throws/day = 38 No. of days = 17 $n = 646$

TARGET													Row Total	
		1	2	3	6	5	4	1	2	3	6	5		
A	1	16	1	13	34	5	16							85
C	2		10	12	42	3	25	9						101
T	3			18	44	7	26	15	8					118
U	6				49	8	19	12	11	12				111
A	5					4	35	12	4	11	45		111	
L	4						31	12	4	10	52	11	120	
Diagonal Total							128	112	97	89	119	101	646	
Target Frequency							4	2	1	2	7	1	17	

SUBJECT C.V. Throws/day = 38 No. of days = 17 $n = 646$

	TARGET												Row Total	
		1	2	3	6	5	4	1	2	3	6	5		
A	1	12	4	15	50	5	26							112
C	2		7	9	43	6	20	9						94
T	3			12	48	7	31	17	7					122
U	6				48	13	19	11	9	15				115
A	5					4	22	14	8	11	40			99
L	4							34	13	3	14	37	3	104
Diagonal Total							117	109	101	120	99	100	646	
Target Frequency							4	2	1	2	7	1	17	

SUBJECT J.B. (1) Throws/day = 100 No. of days = 100 $n = 10,000$

		TARGET											Row Total	
		1	2	3	6	5	4	1	2	3	6	5		
A	1	232	323	256	313	202	276							1602
C	2		343	277	333	218	284	232						1687
T	3			325	327	221	226	247	325					1671
U	6				359	218	248	261	318	287				1691
A	5					241	282	254	277	287	353			1694
L	4						284	274	314	268	315	200		1655
Diagonal Total							1784	1701	1626	1563	1653	1673	10,000	
Target Frequency							16	15	19	17	20	13	100	

SUBJECT A.N. Throws/day = 100 No. of days = 27 $n = 2,700$

		TARGET											Row Total	
		1	2	3	6	5	4	1	2	3	6	5		
A	1	46	92	83	105	56	53							435
C	2		82	80	90	50	82	49						433
T	3			72	89	65	67	54	65					412
U	6				107	82	73	57	82	84				485
A	5					71	67	55	86	92	98		469	
L	4						58	39	93	89	111	76	466	
Diagonal Total							436	449	459	454	477	425	2,700	
Target Frequency							4	3	5	5	6	4	27	

SUBJECT A.I.B. Throws/day = 100 No. of days = 9 $n = 900$

		TARGET											Row Total	
		1	2	3	6	5	4	1	2	3	6	5		
A	1	36	32	24	13	16	24							145
C	2		35	23	18	16	43	37						172
T	3			10	24	14	32	29	38					147
U	6				17	17	24	30	35	21				144
A	5					21	44	35	31	11	15		157	
L	4						33	33	29	11	13	16	135	
Diagonal Total								152	173	144	133	147	151	900
Target Frequency								2	2	2	1	1	1	9

SUBJECT B.R.B. Throws/day = 100 No. of days = 9 $n = 900$

		TARGET										Row Total	
		1	2	3	6	5	4	1	2	3	6	5	
A	1	18	29	16	26	44	28						161
C	2		21	17	13	35	42	16					144
T	3			18	19	25	28	15	27				132
U	6				14	34	39	15	50	18			170
A	5					25	36	22	32	14	11		140
L	4						27	14	41	17	17	21	153
Diagonal Total							123	149	156	153	182	237	900
Target Frequency							2	1	2	1	1	2 [*]	9

SUBJECT J.A.C. Throws/day = 100 No. of days = 100 $n = 10,000$

		TARGET										Row Total		
		1	2	3	6	5	4	1	2	3	6	5		
A	1	380	275	277	228	196	251							1607
C	2		246	248	231	214	272	377						1588
T	3			265	273	241	253	446	249					1727
U	6				294	211	279	422	313	281				1800
A	5					205	268	385	249	259	242			1608
L	4						277	390	268	270	232	233		1670
Diagonal Total							1667	1665	1681	1636	1718	1633	10,000	
Target Frequency							16	24	16	16	15	13	100	

SUBJECT J.B. (2) Throws/day = 100 No. of days = 36 $n = 3,600$

		TARGET										Row Total	
		1	2	3	6	5	4	1	2	3	6	5	
A	1	118	95	104	89	99	89						594
C	2		87	97	115	103	101	89					592
T	3			102	88	95	106	103	87				581
U	6				135	107	86	94	113	92			627
A	5					105	112	98	106	104	88		613
L	4						106	98	112	101	85	91	593
Diagonal Total							653	597	610	599	605	536	3,600
Target Frequency							6	6	6	6	6	6	36

SUBJECT P.A.C. Throws/day = 100 No. of days = 50 $n = 5,000$

		TARGET										Row Total	
		1	2	3	6	5	4	1	2	3	6	5	
A	1	136	119	113	135	129	161						793
C	2		159	122	159	133	186	132					891
T	3			112	134	109	149	107	112				723
U	6				158	151	158	136	145	121			869
A	5					158	186	156	143	128	165		936
L	4						160	133	122	104	149	120	788
Diagonal Total							883	835	827	800	844	811	5,000
Target Frequency							10	8	8	7	9	8	50

SUBJECT J.W. Throws/day = 100 No. of days = 50 $n = 5,000$

	TARGET											Row Total	
		1	2	3	6	5	4	1	2	3	6	5	
A	1	170	98	125	193	203	75						864
C	2		103	101	201	166	81	145					797
T	3			106	189	164	80	144	99				782
U	6				230	212	83	166	101	126			918
A	5					197	99	142	104	131	187		860
L	4						82	133	95	111	200	158	779
Diagonal Total							888	832	810	820	860	790	5,000
Target Frequency							5	9	6	7	12	11	50

A PUNNING AUTOMATISM

REPORTED BY KATHLEEN GAY

A CURIOUS incident occurred on 6 November 1952 when I was having a proxy sitting with Miss Geraldine Cummins, a non-professional medium well known to members of the Society, on behalf of a woman who was unknown to me. The sitting took place with Miss Cummins's hand alone on a ouija board, while I took notes as the pointer indicated the letters on the board. I must add that during the whole sitting no word was spoken by me and no questions were asked by Miss Cummins or her 'Control'.

At the end of the sitting Miss Cummins was very sleepy and her hand had ceased moving, when it suddenly began moving again quite briskly and spelt an entirely fresh message, seemingly intended for me. This was as follows :

Now I get new names, a George and somebody else. I think it is Charles or John, but now he goes off. I see a woman, very bright, who lived to a good age. She is by herself. I get the name of Kit ; very bright eyes and observant. She is at present she says engaged in a conspiracy. She is determined to win in the end. She generally did get her own way this elderly man says. I think she connects up with Cyril.

The names all happen to be applicable except John. Cyril was the name of my husband, who died in 1949, Charles was my

grandfather's name, George and Kit were the names of two of my grandfather's children, and Kit was my mother's name. My mother had very bright eyes and was unusually observant as she was deaf. She died in 1940, and her brother George in 1938. Miss Cummins knows my husband's name, but not that the other names (except John) are those of relations of mine. They are, however, quite common names, and cannot be regarded as evidence of paranormality.

The message continued,

Cyril wants to talk. 'It is good to see you and feel your nearness. I was talking to your father. I must not forget to give you this message. I was talking to him about our fruit farm when he said: "Be sure to tell K. that one pip planted here has burgeoned [Note. The medium paused a moment after the first letter E, as if uncertain how to spell the rest of the word] surpassingly well, and is it not surprising in view of Pip's past history?"' I pass on this message but said it did not make sense, and he replied: "If K.'s intelligence has not withered with age she will understand." I think it is nonsense. Your father does not know a thing about growing apples. I was enjoying telling him about the young trees I had planted, but he interrupts with this rubbish about growing a tree from a pip and keeps laughing. I don't see the point, much less the joke, but it is hardly fair to keep me out of it. This quite passes me by and I don't see why I should be practised on with old family jokes that mean nothing to me.' [Here the sitting came to an end.]

To understand the unintelligible joke which this passage contained, it is necessary to explain that my grandfather, Charles Burge, was always called 'Pip' by his children. He was a rather self-indulgent person who gave himself up to the pleasures of this life to an extent which incurred the mild disapproval of my father during his lifetime. 'Pip' Burge died in 1917 before my marriage. I had not thought of him for years and he meant little to me. My husband had heard me speak of him but never knew him, and I doubt if he ever heard his nick-name, which was not used by me or my generation.

Why this curious allusion to a forgotten person should have appeared, given in a form which was quite alien both to my conscious mind and that of Miss Cummins, I am at a loss to explain. I do not think there is anyone now living outside my family who could know this nick-name, which is not an abbreviation (of Philip, for instance) but purely a nick-name. Miss Cummins knows none of my relations and could not possibly have heard of him. It can, of course, be said that I might on some occasion have mentioned him to her and that this incident was merely a camouflaged emergence of a forgotten memory; but I am con-

vinced that I have never spoken of him to her or in her presence. I have had no occasion whatever to speak to her of my pleasure-loving ancestor, in whom she would not have the slightest reason to be interested. It can also be said that a trivial memory was obtained telepathically from my subconscious mind and dramatised in the form of a conversation between my father and my husband, with a pun thrown in. Whatever the explanation, the episode seems worthy to be placed on record.

I should perhaps add, in regard to the reference to the growing of apples, that my husband and I had a fruit farm before the War. As evidence, the reference must be discounted. I probably mentioned it to Miss Cummins or to her friend, the late Miss Gibbes, who was a keen gardener, and she might have told Miss Cummins.

G. N. M. TYRRELL AND HIS CONTRIBUTIONS TO PSYCHICAL RESEARCH

George Nugent Merle Tyrrell, who died on 29 October 1952, was born in 1879 and was educated at Haileybury, Seafeld Engineering College, and London University, where he took a degree in Physics and Mathematics. He was one of the early students of wireless under Marconi, and when he joined the Society in 1908 he was in Mexico, demonstrating the working of the Marconi system to the Government. He served in the First World War as signals officer in the Royal Artillery, and was twice mentioned in despatches.

After the war psychical research grew to be the main interest of his life. In 1921 he conducted a short series of experiments with Miss Gertrude Johnson. She had been known to him and Mrs Tyrrell for several years and later became a member of their family. Tyrrell often discussed his projected books with her, and greatly valued her advice. In the report of the experiments printed in the *S.P.R. Journal* for June 1922 Tyrrell is called 'Mr. T.' and Miss Johnson 'Miss Nancy Sinclair'. It is doubtless because the real names were not given at the time that in the great interest aroused by Tyrrell's later experiments with Miss Johnson this earlier work has often been overlooked.

The report throws much light on the way Tyrrell's interest in psychical research developed. His was not the sort of mind that would in any circumstances have contented itself with a narrow view of its objects and methods, but any tendency in that direc-

tion, had it ever existed, would have been checked by the variety of Miss Johnson's phenomena. She wrote automatically in trances of varying intensity; she had spontaneous flashes of apparent telepathy and precognition of the kind that create an impression no rational observer would disregard, even though they could not be quoted as evidence; and she saw visions in a crystal.

The most interesting part of the report, however, relates to experiments with playing cards. In some of the experiments Miss Johnson read off the values of the top cards of an already shuffled pack; in others she would specify a series of cards, and on a later day, the cards having been shuffled by Mr or Mrs Tyrrell, her forecast would be checked. With either method her scores enormously exceeded chance probability.

In experiments within the family circle there is often some informality which invites criticism that the conditions may not have been up to laboratory standards. In his later experiments Tyrrell was at pains to forestall objections of this sort by efforts to perfect the methods used, and particularly by devising apparatus of various kinds, referred to below, but the significant point of the 1921 experiments is that they were quantitative. Several of the experiments conducted in the early years of the Society were of that nature, but for many years before 1921 the attention of psychical researchers had been almost entirely turned in other directions. In reviving, therefore, interest in quantitative experiment Tyrrell may be regarded as the pioneer of a form of research which has since become of primary importance both in this country and in the United States.

For several years, however, circumstances did not permit the continuance of the experiments with Miss Johnson. By the time Tyrrell resumed them in 1934 quantitative research was well established. In 1928 Miss Jephson's important paper on Clairvoyance had been published (*Proceedings*, XXXVIII) and in 1934 appeared Professor Rhine's *Extrasensory Perception*. The main problems now facing Tyrrell were these: to devise an experimental technique adapted to Miss Johnson's powers in such a way as to elicit the maximum of exact knowledge of them; to ascertain, as the Council invited him to do in 1935, whether the results obtained with her could be repeated with other subjects; and to explore methods of differentiating between different types of ESP. With these objects in view he devised first the pointer apparatus and then the electrical apparatus mentioned below by Mr G. W. Fisk, who was closely associated with his experiments.

In his Presidential Address, delivered in June 1945, Tyrrell spoke of the danger of psychical research splitting into two groups

of statistical and qualitative workers, and added: 'It is unnecessary that there should be this division, but it is likely to occur because two different types of mind and two different temperaments are involved . . . we need both types of mind in psychical research. Unfortunately, they are seldom united in the same individual.' Fortunately, they were united, so far as that is possible, in Tyrrell. He gave abundant proof of this when, having been appointed Myers Memorial Lecturer for 1942, he took the opportunity of making a much-needed review of the subject of apparitions. These had provided the material for the first enquiry on a large scale undertaken by the Society, and in 1886 the results were published in *Phantasms of the Living*. In 1923 Mrs Sidgwick made an important collection and analysis of cases printed by the Society since 1886, but a review of the theoretical implications of this kind of phenomenon had long been overdue. On this subject Tyrrell's lecture *Apparitions* is likely to remain a classic for many years.

Professor Price is contributing a discussion of the theoretical and philosophical aspects of Tyrrell's work, and I gladly leave those topics to him, but any tributes to Tyrrell would be incomplete without mention of his outstanding ability as an exponent of psychical research to the general reader. The letters received by the Society tell a melancholy story of intelligent inquirers making their first acquaintance with psychical research through literature crammed with unverified and unverifiable 'facts' loosely strung together to support a sensational conclusion. It is useless to invite the inquirer to garner the truth for himself from the 49 volumes of our *Proceedings* and the 36 volumes of our *Journal*. It is accordingly an immense relief to be able to refer him to comprehensive surveys of the evidence as well arranged and as acutely discussed as Tyrrell's *Science and Psychical Phenomena* (1938) and the Pelican, *The Personality of Man* (1946). Nothing as good in their line as these books had appeared for many years, not, I would say, since the publication of Myers's *Human Personality* (1903). In style they well reflect the cast of Tyrrell's mind, modest, thoughtful, quietly humorous.

W. H. SALTER

EXPERIMENTAL WORK

I first became acquainted with Mr Tyrrell and his experimental work in 1935. He had invited me to act as experimenter and agent with Miss Johnson to see if she would achieve significant results with agents other than himself. I was very glad to do so. Trials

were made with ESP cards and also with the 'Pointer apparatus'. Both methods yielded significant results, although the deviations from chance expectation were somewhat smaller than those obtained when Mr Tyrrell was in control.

Perhaps I should explain for the benefit of those who are unacquainted with Mr Tyrrell's unique contributions to ESP experimental research that he initiated the technique of a subject 'finding' an object, under controlled conditions, in lieu of 'guessing' a design or pattern with ESP cards or 'identifying' drawings, etc. He had noticed that Miss Johnson had a flair for finding lost objects in the home and designed apparatus to test that natural aptitude. The Pointer apparatus was an early model. It was quite simple, and consisted of five small boxes with hinged lids mounted behind a board in such a way that a pointer could be thrust through holes in the board into any selected box without the subject being able to see what the operator was doing. The subject was required to find, by raising the lid, the box into which the pointer had been inserted, the probability of success being, of course, one in five.

Two main lines of criticism were directed against the Pointer apparatus technique. There was the possibility of sensory clues being given to the subject—such as some sound being made by the pointer entering a box no matter how carefully it was done and how thoroughly the surfaces were padded. There was also the possibility of the operator's and subject's number habits coinciding and so producing a fictitious score. But Mr Tyrrell was his own chief critic. He was always weighing in his mind every possible source of error; ever ready to listen to any criticism or suggestion, balancing the pros and cons with his own fine judicial mind. He would even listen patiently to the half-baked criticisms of a tyro like myself and then would interrupt my halting exposition with a 'Don't you mean this?' and proceed to elaborate my point with that lucidity of thought and utterance that were so habitual with him. I recall how on one occasion when I was able to demonstrate a method of wangling a false score with his later electric machine, he became as excited as I was and congratulated me with enthusiasm. He then immediately set to work to think the matter out, and eventually to modify his technique so as to make such a wangle impossible. That gave me an insight into his attitude as an experimenter which I have never forgotten.

The original Pointer apparatus was soon greatly elaborated and improved. The five boxes were retained, but each box was fitted with a tiny electric lamp which glowed when a corresponding key was pressed by the operator. The number of trials, successes, and

failures were recorded automatically on a paper tape. The electric wiring allowed the separation of operator and subject by any required distance. From time to time, as suggestions were made, other refinements were introduced. No one could have been more eager than Mr Tyrrell to adapt the apparatus to meet even remotely plausible criticism. Thus the final machine had, among many minor improvements, (a) a special switch that jumbled the wires from the keys so that the operator could not know which lamp was lighting when any one of the five keys was pressed, (b) a delayed action relay so arranged that no lamp would light up until *after* the subject had actually raised the lid of the correct box although the circuit determining which lamp was involved had already been fixed by the apparatus, (c) a mechanical selector which determined the operation of the keys in a random order.

Miss Johnson continued to score significantly above chance expectation no matter what alterations were made in the apparatus, although with the introduction of any change the score generally fell off for a longer or shorter period, until the psi faculty, as it were, became used to the new conditions.

So Mr Tyrrell had thus devised and cut a new and intricate key with which to test the locks of the closed doors of knowledge. One could reasonably hope that new light would continue to be shone on the conditions that determine the emergence of psi phenomena. Then the war broke out and one of Hitler's bombs smashed everything. But it was not altogether a misfortune, for it meant that the experimenter would retire and yield undisputed place to the author and philosopher. That bomb gave Mr Tyrrell time to write his paper on Apparitions and the books which are now known and studied all over the world.

G. W. FISK

THEORETICAL AND PHILOSOPHICAL ASPECTS

Tyrrell conceived of *Psychical Research* as 'the exploration of human personality', and he was deeply impressed by the *queerness* of the conclusions to which this exploration seems to commit us. He thought they were queer not only in detail (in the way that the duck-billed platypus is a queer animal) but in principle, because they just would not fit at all into our current world-outlook. In order to make sense of them—indeed, in order to get the facts themselves accepted—a kind of intellectual revolution would be needed. This is a recurrent theme in all his writings.

Is there any prospect of such a revolution? On this point we can trace a certain ambivalence of view in Tyrrell's work, an

oscillation between hope and despair. The pessimistic view, most clearly stated in *Homo Faber*, is a kind of Kantian one, though it is also influenced by Tyrrell's study of Bergson and Bradley. In this mood, he maintains that the queer facts discovered by Psychological Research merely reinforce the conclusions to which reflection on normal experience ought in any case to have brought us, namely that our ordinary beliefs about the world and about ourselves are based on biologically-useful illusions. The function of the human intellect, as of the human senses, is merely to adjust *homo faber* to his environment. The chief illusion is the biologically-instilled assumption that the perceptible world is absolutely real and independent of ourselves, whereas in fact it is only a phenomenal world, distorted and artificially simplified by the character of our perceptual and intellectual faculties. Not only the sensible qualities we seem to find in it, but also the principles of order it seems to display—space, time, matter, causality—are imposed upon it by 'the adapted mind'. Of course there *is* a Reality—a real universe and a real self—lying behind the phenomenal world. Tyrrell never doubts this, any more than Kant did. But what that Reality is, the human mind can never grasp. In particular, human personality, as it *really* is, is something 'incomprehensible', 'beyond our understanding'. We simply have not got the concepts which would be needed for making it comprehensible, and we have no means of acquiring them.

It is true that even when Tyrrell takes his pessimistic line, he does always admit that there is a mode of awareness, in all men or at least in some, which transcends the limitations and the illusions of the 'adapted mind'. In mystics, in men of genius, in poets, and even occasionally in the ordinary man, there are intimations of something other than and deeper than the phenomenal world. But in his pessimistic mood, Tyrrell seems to think that this awareness, which raises us occasionally above the biologically-conditioned myopia of *homo faber*, is something purely intuitive and incapable of being conceptualised; something which can express itself at the best only in symbols and in metaphors, and is therefore useless for the purpose of constructing a theory, or an explanation, or anything approaching a theoretical understanding of human personality. Consequently there could never be a *science* of Psychological Research; or if there could, the most it could do would be to establish that supernormal events occur, and it could never do anything to make them intelligible.

This theory has its difficulties. The most important one arises from the conception of 'adapted mind' itself. All the evidence we have for the proposition that living creatures are adapted, mentally

or physically, to their environment is evidence which comes from within the world which sense-perception discloses. But if this world is merely phenomenal, not absolutely real, adaptation itself is merely a phenomenal relation between phenomenal entities. In the world as it really is, there are neither organisms nor an environment, though in the world as sense-perception discloses it there appear to be both. We note too that according to Tyrrell the 'adapted' character of the human mind is caused by an evolutionary process. But according to his own theory the concept of cause only has application *within* the phenomenal world. Evolution too must surely be a temporal process. But according to the theory, the concept of time applies only to phenomenal entities; the real world is non-temporal or super-temporal. It would seem that Tyrrell is using Realistic premises to establish an Idealistic conclusion. In his premises, he asserts that the human mind *really is* adapted to an environment which it *really has*, and that this adaptation *really was* the result of an evolutionary process. But his conclusion commits him to the view that the evolutionary process, the environment, and even living organisms themselves are merely phenomenal, appearances (and misleading appearances) of a Reality whose intrinsic nature is unknown to us. To put it in Kantian language: in his premises he is claiming a knowledge of 'Things in Themselves' (a *trans*-phenomenal knowledge), which he cannot possibly have if his own Idealistic conclusion is true. And this knowledge cannot be just an intuitive awareness, incapable of conceptual formulation; it must be sufficiently conceptualised to serve as the basis of an elaborate philosophical argument.

But this quasi-Kantian theory, with the pessimistic conclusions about the prospects of *Psychical Research* which follow from it, is only one strand in Tyrrell's thought; and even if he had stuck to it wholeheartedly, his own constructive work as a psychical researcher would be the best refutation of such an agnostic philosophy. *Solvitur ambulando*. He shows that 'the exploration of human personality' is a possible enterprise for the human intellect by actually doing it. He shows us that illuminating explanatory concepts can be produced by actually producing them. He shows us that the mysterious depths of human personality are *not* altogether 'beyond our understanding' by actually helping us to understand them a good deal better than we did before. First, he points out that human personality, when seen in the light which supernormal phenomena throw upon it, turns out to be something vastly more complex than we had supposed. We should conceive of it as a 'many-levelled' or 'many-graded' hierarchy. As he sometimes says, there are 'degrees of I-ness' in it. For example, a

man's instinctive desires are 'more I' than his body, and his body is 'more I' than his clothes. What is 'most I'—the Pure Ego of the philosophers, the Atman of the Hindu mystics—may indeed remain incomprehensible to the end. But we can hope for some comprehension of the intermediate levels which lie between the Pure Ego at the one end and the material organism at the other—provided we try to form ideas which will fit the facts instead of forcing the facts to fit our preconceived ideas.

Those considerations led Tyrrell to suggest that supernormal cognition occurs in two distinct stages. The telepathic contact between two personalities is something which occurs, or exists, at the unconscious level; and then, in addition, the results of it have to be 'signalled' to consciousness. Tyrrell has interesting things to say about both stages. The first, though one may use words like 'contact' and 'reception' to describe it, is not to be thought of as a relation between two personalities which were initially separate, and the spatial prefix *tele-* is misleading. Nothing 'passes' or 'is transferred' from the one mind to the other, because at the deeper levels of the personality there is no separation between mind and mind at all. In the second stage, the crucial process is one of imaginative dramatisation, which may take many different forms, and sometimes highly symbolic ones. Moreover, what is telepathically received is not a cut-and-dried 'message', but a relatively general idea or 'theme'; and this has to be worked out in detail (and perhaps distorted in the process) by the unconscious imaginative powers of the recipient, humourously personified under the names of 'the producer' and 'the stage-carpenter' in the lecture on *Apparitions*. This process of detailed imaginative dramatisation is seen at its best in the production of full-blown telepathic apparitions, complete with apparitional clothes and other apparitional appurtenances (e.g. horses, carriages) to suit them. But something like it, this time in a verbal form, occurs in mediumistic communications too. Tyrrell draws two important conclusions from this. On the one hand, no mediumistic communication can be taken at its face value. It is not at all like a communication by telephone. On the other hand, the two rival explanations of the facts, the Survivalist one, and the Naturalistic one in terms of secondary personalities, are not so sharply opposed as they look. Who or what is communicating? Is it a discarnate personality, or is it just a secondary personality of the medium? The answer may be that it is a temporary and *ad hoc* personality which is a blend of both; and the two constituents of it might be blended in different proportions in different cases. As he says at the end of the lecture on *Apparitions* (p. 122) his personal

impression of the work which has been done on the problem of survival since 1882 is that the crude question whether we survive death 'has been rubbed off the slate . . . and instead of a direct answer, we have had revealed to us something of the general perspective in which the question ought to be asked'. This impression seems a very just one. But it should be added that Tyrrell's own work has contributed very notably to this revelation of the appropriate 'general perspective'.

If we consider the constructive suggestions which Tyrrell himself has made, we can see why the deeper levels of human personality, and the supernormal occurrences which have their source there are liable to appear 'incomprehensible'. The fault does not lie with *homo faber*, but with Descartes. The 'incomprehensibility' of the phenomena is not due to any inherent limitation of our intellectual powers, as Tyrrell, in his pessimistic mood, supposed it was. It is something historically conditioned, a result of the climate of opinion which has prevailed among educated Western Europeans from the middle of the seventeenth century to our own day. Descartes did more than anyone to establish this climate of opinion, and his theory of the human mind was a peculiarly narrow and rigid one. A human mind, he thought, is an indivisible psychical substance whose essential attribute is consciousness. From this it follows that the concept of unconscious mental activity is a self-contradictory one, and it is likewise self-contradictory to suppose that a human personality might split into two or more sub-personalities. Moreover, he thought that a human mind is causally insulated from all other finite minds; the only causal relations in which it could conceivably stand were relations with its own brain and with God. From this it follows that telepathy is a self-contradictory conception, and of course psychokinesis also. Though later philosophers protested against this Cartesian theory, it came to be accepted as part of Western European 'common sense'. It is these inherited Cartesian preconceptions which have made the phenomena of Psychical Research appear incomprehensible; and indeed the phenomena of Psychopathology too, though the educated public is willing to put up with these, under protest, because it has turned out that the investigation of them has practical therapeutic value. We must get rid of these Cartesian preconceptions if we are ever to make any progress with the theoretical side of our subject. It is very difficult to get rid of them, and still more difficult to think of something more adequate to take their place. But it is not impossible, as Tyrrell's own constructive work has shown.

H. H. PRICE

REVIEWS

WE SAW HER. Translated and arranged by B. G. Sandhurst, with an Introduction by C. C. Martindale, S.J. London, Longmans, Green, 1953. xx, 226 pp. 11 plates. 12s. 6d.

The purpose of this book, as explained by the author, is to enable his readers to study the evidence of eye witnesses of the events which took place at Lourdes in 1858. He writes as a believer, and frankly disavows any attempt to affect impartiality (p. 43). This is evident from the manner in which he selects from the large body of evidence which is available, and his handling of what he has selected (cf. App. to xii on p. 195). Nevertheless, the book is of interest to the student of psychical research as illustrating the immense difficulties which confront the investigator of spontaneous cases.

The 'subject' here was a French girl of very poor family, about 14 years old, and probably in a state of semi-starvation at the time of her 'experience', which began on 11 February 1858. The original experiences lasted, with intermissions, till 4 March. Within that period, it is necessary to distinguish the original vision from the later phases of the experience, after well-meaning grown-ups had intervened in an endeavour to identify the apparition. Bernadette herself, in a statement written by her about three years after the experience, by which time she had learned to write, said simply, 'I saw a lady dressed in white. She wore a white dress and a blue sash and had a yellow rose on each foot, of the same colour as the chain of her rosary' (p. 49). In an earlier account, reported at second hand by the schoolmaster Clarens, Bernadette is quoted as having said she had seen 'a girl in white', and gave some details of her dress which differ in one or two unimportant respects from those given in her own written statement referred to above (pp. 51-2). When later on she was questioned by Inspector Jacomet, he actually put it to her, 'You say it is the Blessed Virgin who has appeared to you?' and Bernadette replied, 'I do not know if it is her. She has not told me' (p. 95). When she first mentioned the original vision at home, her mother thought it might be the soul of one of their relations in Purgatory. Another local woman, Antoinette Peyret, a sewing-maid, thought the white lady might be a certain Elise Latepie, who had died a month before (pp. 69 and 93). Naturally this sort of theory melted away rapidly in the climate of Lourdes, and another identification took its place.

The earliest recorded suggestion of identification with the Virgin occurs in a version of Bernadette's story as told to the Prosecutor, M. Dutour on 21 February, ten days after the original

vision. Describing the vision, she is then quoted as saying, 'Her face and clothes reminded me of a [statue of the] Blessed Virgin before which I used to pray, but she was alive and surrounded with light' (p. 57). Mr Sandhurst rates very low the reliability of M. Dutour as a witness, and thinks that the account, from which the above sentence is quoted, was possibly composed long after the event by the Prosecutor, who was hostile on 21 February 1858, and only became a believer much later on. It would seem, then, that the identification of the apparition with the Virgin was not based on any statement by Bernadette made during the original experience, but was at first inferred by those around her from signs which convinced the faithful but do not constitute evidence for others. It was not until 25 March, by which date the conviction had doubtless communicated itself to Bernadette, that she reported an announcement by the vision that she was 'the Immaculate Conception'. This, like other later developments following upon the original experience, must be left to the student of religion.

It is perhaps not out of place here to refer to the question of the origin of Bernadette's experience. On this point the author, in his Introduction to Part II (p. 43) resorts to a favourite device of the controversialist, and attempts to force the reader on to one of the horns of a dilemma. 'The open-minded reader is faced with the question of whether Bernadette's visions were real, and of divine origin, or whether she was an *hallucinée*. There is no third alternative.' The cases collected by this Society over a period of some 70 years show that hallucinations are by no means all 'mere hallucinations', and that many persons whose sanity is undoubted have had hallucinations of a veridical character. But the question, for a psychical researcher, whether a given hallucination is veridical or not is one that can only be answered by a careful sifting of the evidence on scientific lines. For such an inquirer there is no dilemma of the kind suggested above.

For an earlier account of the Lourdes legend see an article by F. W. H. Myers in *Proceedings* (Vol. ix, pp. 176-82) quoted in *Human Personality*, Vol. I., Appendix 578A. In that article he refers to *Proceedings*, Vol. vii, p. 100, where a description will be found of a series of apparitions of the Virgin, seen by many persons, near Pontinet in the Dordogne. In that case the visions were discouraged by the Bishop, and nothing came of them.

The book has an 'Index of Characters' (but no index of subjects), and contains some interesting illustrations.

G. W. L.

THE GOD OF THE WITCHES. By Margaret Alice Murray, D.Lit.

New edition. London, Faber, 1952. 212 pp. 16 plates. 21s.

In a foreword to this second edition it is stated that the original matter remains unaltered, but that additions include an account of the Puck Fair of Co. Kerry, the whole of Chapter VI (The Position of the Witch in the Social Structure), the meaning of the 'Face of Lucca' in the oath of William Rufus, and an illustration of a fifteenth century miniature painting which shows Joan of Arc identifying the Dauphin. Dr Murray maintains that the witch cult must be regarded as a religious cult, on a par with other religions, and that it can be traced back to Palaeolithic Man. In support of this thesis she marshals massive evidence accompanied by persuasive arguments, which she expresses in so charming a style that her great erudition is nowhere oppressive.

Most of the evidence for the first contention is obtained from accounts of the accused in seventeenth century witch trials. Thus, on p. 33, we have, 'Of the Essex and Suffolk witches, whose trials made such a stir in 1646, Rebecca West "confessed that her mother prayed constantly (and as the world thought, very seriously) but she said it was to the Devil, using these words, *Oh my God, my God*, meaning him and not the Lord."'

The witch cult is considered to be the religion of a primitive folk, the Fairies, preserved by aural tradition (they were illiterate) from prehistoric times. The idea that a Fairy is of much less than human size is attributed to Shakespeare. Much information about the fairy folk is given, and many suggestions are made to account normally for the supposedly magical.

The book is well reproduced and the sixteen illustrations are excellent, the one slight defect being that the index is too incomplete to serve its purpose easily. For example, on p. 48 are mentioned Shakespeare, Goodfellow, Oberon, Keightly, Hobgoblin, Merry Wives of Windsor, Anne Page, John Walsh, Bessie Dunlop, and Elfane—none of which is to be found in the index. There is, however, an extensive bibliography.

The reviewer hopes that those members of the Society who have not read *The God of the Witches* will now do so, and that they will enjoy it as much as he has enjoyed it and been instructed by it.

C. C. L. G.

CORRESPONDENCE

METAPHYSICAL APPROACHES TO PRECOGNITION

SIR,—In the November-December 1952 issue of the *Journal* Professor N. O. Lossky has suggested that his 'Hierarchical Personalism' provides us with the kind of metaphysical perspective we need in investigations of ESP and PK. I have recently argued elsewhere¹ that the Mystical Intuitivism of Vladimir Soloviev (1853-1900) and his distinguished philosophic successors, of whom Professor Lossky is one, is entitled to the serious consideration of metaphysicians who are anxious to speculate on the far-reaching implications of psychical research. Professor Lossky has opined that the hypothesis of super-temporal, super-spatial, substantival agents can accommodate precognition. I agree with him that a metaphysical hypothesis of this kind is far more promising than many current theories of time based on current denials of psi phenomena. Classical Russian philosophy contains the valuable suggestion that there may be an unfamiliar 'higher time' appropriate to the inner lives of Spiritual Selves. S. A. Alexeyev (Askoldov, 1870-1945) observed that the scientific emphasis on time-measurement and linkage with a metrizable space obscures the ontological status of Persons who belong to a 'Kingdom of Spirit'.²

I wish to point out, however, that metaphysical hypotheses cannot serve as working hypotheses of psychical research unless they maintain the closest touch with the investigation of experimental and spontaneous psi phenomena. Specific problems await discussion. For instance, does transcendence of our familiar time mean that precognition has no limits at all? Can a person precognize events which come to pass after his death in empirical time? We know that H. F. Saltmarsh raised this problem and suggested that it has a bearing on the whole problem of survival (*Proceedings S.P.R.*, February 1934, p. 93, last paragraph). I have the impression—I may, of course, be wrong—that the literature on spontaneous precognition, ill documented as most of it undoubtedly is, contains suggestions for possible answers to these metaphysical puzzles.

¹ 'Russian and Indian Mysticism in East-West Synthesis' in *Philosophy East and West* (Honolulu, University of Hawaii), October 1952.

² See the interesting references to Alexeyev in Lossky, *The World as an Organic Whole* (Oxford University Press, 1928), pp. 88-90; *History of Russian Philosophy* (New York, International Universities Press, 1951), p. 382.

Again, on some metaphysical hypothesis, can we say that the situation in which precognitive ESP is likely to emerge is equally a situation in which retrocognitive ESP is likely to manifest itself? I have remarked in the *Journal* (November-December 1951) that the clustering of 'fore-hits' and 'back-hits' round the 'targets' seems to characterize some of the spontaneous as well as the experimental cases. G. W. Fisk's 'Home-Testing ESP Experiments' (*Journal, op. cit.*) have furnished some evidence for significant *negative* scoring on both +1 and -1 displacements. The suggestion that a 'fore-hit' and a 'back-hit' may *occur together* with the same subject in the same trial, i.e. that ESP calls may be 'multiply determined' or 'reinforced', is more plausible now than it was when it was first advanced by Soal. When M. S. Bartlett observed (*Proceedings*, April 1949) that the χ^2 -test of deviation could not be reliably applied to the 'multiply-determined' hits, Soal admitted the force of the criticism. But the evaluations made since then by J. G. Pratt (*Journal of Parapsychology*, June 1951) and D. J. West (*Journal S.P.R.*, January 1953), on the basis of independent methods of assessment proposed by T. N. E. Greville and H. Robbins, have shown that the evidence for the 'multiply-determined' hits is not negligible. I am inclined to suppose that the effect, which the late G. N. M. Tyrrell called the 'sandwich' effect (*The Personality of Man*, p. 125), is just what we should expect if the cognition at work in the ESP situation were to cut directly across two events, one relatively 'past' and the other relatively 'future', separated in our ordinary time, i.e. if it were to perceive their likeness in a single 'associative act'. One is reminded of the characterization of ESP as a 'diametric' rather than a 'circumferential' function (J. B. Rhine *et al.*, *Extrasensory Perception After Sixty Years*, pp. 316-19). Investigators of spontaneous and experimental precognition must watch for and report the phenomenon. I have suggested that some metaphysical reformulation of Saltmarsh's hypothesis of a 'prolonged subliminal specious present' may provide approaches to specific problems. I should like to make it clear that any reformulation would mean our explicitly challenging current assumptions about the 'specious present' and about temporal characteristics generally; it may mean our going far in the direction of theories of the kind propounded by Alexeyev.

The significant precognitive 'misses' seem to call for a 'depth psychology' of some kind. One is tempted to seek a crude analogy to the situation in the 'screen memories' which psycho-analysts claim to have encountered in their practice, i.e. memories of events which never happened but which cloak, sometimes in highly

specific ways, what really *did* happen.¹ The following spontaneous case, which I investigated in some detail, may perhaps serve to make my suggestion more definite. I was staying with Mr and Mrs J. (pseudonyms). Mr J. is related to me; in fact, I am a familiar member of the household. On the morning of Monday, 20 January 1941, at about seven o'clock, soon after waking up, Mrs J. related a dream to me and Mr J. and predicted confidently that a parcel would arrive by post in the forenoon and that it would be placed in the hands of her son, Master J. She did not seem to know who had sent the parcel. I made notes of the dream and the prediction. I may as well say that it was not Master J.'s birthday nor anywhere near it. The circumstances in which the prediction was fulfilled on the same day before noon suggested to me—one has to risk an opinion in these cases—ESP. The parcel was *addressed* to Mr J.; there was nothing on the covering label indicating that it was really *intended* for Master J.; the label just said 'Books with Care'; Mr J. frequently gets books by post; but this parcel contained a 'Wonder Book of Working Models' advertised by a well-known 'Home Library Club'. By a twist of circumstances, it actually found its way to Master J. at school without Mr or Mrs J. intervening. The person who had placed orders with the publishers for the despatch of the parcel was a friend of the family, Mr R. (not a pseudonym); it was the only parcel of that description that he ever ordered to be sent to the J.'s. I subsequently learnt from my sister Mrs S. (not a pseudonym) the specific circumstances in which it had been sent. But here is the curious part of it. When Mrs J. narrated her dream, I suggested that Mr J.'s brother might have sent a parcel to Master J.; knowing the family as I did, he seemed the likeliest person. But Mrs J. was emphatic (there was a ring of conviction in her voice) that it was *not* he. She seemed to know pretty definitely who had *not* sent the parcel. A prolonged analysis, which I carried out later, made it plausible (I am basing my suggestion also on other ostensibly 'paranormal' phenomena that I witnessed with Mrs J.) that there had been a certain 'resistance' to the emergence of Mr R.'s name into the ordinarily introspectable consciousness.

The hypothesis, which can be extracted from both psychoanalysis and Lewin's 'vectorial psychology', that the way in which 'barriers' are met has much to do with the shaping of our ordinary conscious personalities, demands scrutiny from a new angle. I am far from claiming that it is the 'open sesame' to all the riddles

¹ See, for instance, A. A. Brill, *Fundamental Conceptions of Psychoanalysis* (Allen & Unwin, 1922), Ch. III, pp. 70-5.

of psychical research. I am only saying that a 'dimensional analysis' which scouts all psychologies of not ordinarily introspectable mental ('subliminal', 'subconscious', 'unconscious', etc.) processes as *ad hoc* hypotheses and prefers to interpret Human Personality as the intersection of a number of quantitatively observable variables—for a persuasive exposition of the method, see H. J. Eysenck's *The Scientific Study of Personality* (Routledge & Kegan Paul, 1952, Ch. I)—is not likely to go far in psychical research. Some 'deep analysis'—and I am not thinking only of the orthodox psycho-analytic lines of Charles Berg's *Deep Analysis* (Allen & Unwin, 1946) or of Edward Glover's dichotomy *Freud or Jung* (Allen & Unwin, 1950)—of the ESP percipients and the ESP experimenters is needed. Not only must we tackle the 'sheep' and the 'goats' who figure as successful ESP subjects, but also the Rhines, the Caringtons, the Soals, the Thoulesses, the Fisks, who successfully organize, supervise, and conduct ESP experiments. Judging only by my experiences of spontaneous psi, I should say that the kind of person one is, ~~on a~~ 'deep analysis', may have far more to do with the kind of psi episode in which one is involved than appears at the first blush. Dr Soal has furnished startling evidence for a 'temporal displacement' in card-guessing experiments. May I remind psychical researchers that it was also he who reported the remarkable 'Gordon Davis' case (*Proceedings*, 1926, Part 96), suggesting to me at least 'telepathy à trois' (see Andrew Lang, *Proceedings*, Vol. XV, pp. 48 *et seq.*) and precognition? Did Mrs Blanche Cooper (see Dr Soal's article in *Prediction*, December 1937, pp. 497–8) achieve anything like that either before or after Dr Soal had his sitting with her? Without a 'deep analysis'—and it would have to be frankly extrapolatory in the first instance—the method of 'dimensional analysis', in the field of psychical research, may leave on our hands an odd collection of statistical facts indicating puzzling and varying *directions* in which psi functions or does not function, but little more.

Philosophers speculating on the implications of parapsychology must make a strenuous attempt to relate their theories to concrete data and indicate some lines along which the researches can be profitably pushed further. Notwithstanding the pioneering efforts of Professors Broad, Price, Ducasse, and Lossky, there is still lacking that intimate relation between philosophy and psychical research which is so necessary in the interests of both. Many professional philosophers unfortunately continue to display a regrettable lack of interest in our Society.

Department of Philosophy,
Madras Christian College.

C. T. K. CHARI

'SURVIVAL AND THE IDEA OF "ANOTHER WORLD"'

SIR,—I should like to make one or two comments on those pages of Professor H. H. Price's fascinating exploration 'Survival and the Idea of "Another World"' (*Proc. S.P.R.*, Vol. L, Part 182, pp. 9-11) which were directed at a paper of mine called 'Death' (*University*, Vol. II, No. 2).

(1) My slogan 'People are what you meet' is certainly defective in both the ways he points out: 'people' is not straightforwardly the plural of 'person' (since it lacks important nuances which 'person' has): and no one can in a literal sense meet himself. But the basic point which I was making, and which I used this slogan to mark, remains unaffected. That was that 'Words like "you", "I", "person", "somebody", "Flew", "woman"—though very different in their several particular functions—are all used to refer in one way or another to objects (the pejorative flavour of this word should be discounted) which you can point at, touch, hear, see and talk to. Person words refer to people.'

(2) With great skill Professor Price succeeds in indicating a conceivable mode of existence of possible conscious but incorporeal beings. But this—though a considerable achievement—is not by itself sufficient to show that *we* might become such beings after death, that death for *us* might be a metamorphosis from a substantial to an insubstantial mode of existence. In addition it would be necessary to show that it would be reasonable, if certain conditions were satisfied, to *decide* that particular incorporeal beings could be identified with, could be said to be the same persons as, particular human beings. The word '*decide*' is crucial here. The meanings our person words and expressions at present have are meanings adapted to the needs and facts of this world. We cannot apply them to the radically different possibilities of another world without, tacitly or explicitly, *deciding* to make drastic alterations in their use, their meanings. Logically prior to any possible factual issue 'Is this incorporeal being Myers?' is the decision issue 'Under what conditions, if any, shall we be prepared to extend the meaning of expressions such as "same person" in such a way that it will be significant to say that an incorporeal being either is or is not the same person as Myers?' This may sound a tortuous method of making a trivial point. But the point only sounds trivial in the context of a speculative discussion in which it is always open to us so to arrange our suppositions about possible beings that it would be obviously reasonable to extend our notions of person and personal identity to include the beings in question. But—as Carington long ago pointed out (in *The Meaning of*

'Survival', 1935) the facts of any actual other world may be such that we should not want to decide, even in the light of the fullest knowledge, that a particular insubstantial being either was or was not Myers. We might not want to (feel happy to) extend and apply our notions of person and personal identity here at all. It seems to me that Professor Price, in speaking about what it might be like *for us* in an image world, takes these vital decisions for granted: though of course many of his arguments could be deployed almost unchanged as arguments in favour of making the decisions in the sense in which he has tacitly made them.

(3) Professor Price writes (p. 10) 'And surely the important question is, what constitutes my personal identity *for myself*'. I am not sure I understand his point here. A person either is or is not the one who robbed the bank: he cannot be one thing *for himself* and another *for other people*; though some (usually *but not always* including himself) may be in the secret while others are not. It looks as if Professor Price has momentarily overlooked the fact that in questions about the identity of a person the honest testimony of the person in question—though always important and often decisive—does not necessarily constitute the last word. Whereas in questions about whether or not a person is in pain what that person says, if he is not lying, is *necessarily* decisive, in the face of no matter what medical evidence about the intact condition of his body. It is because this is so, because it is possible to be *mistaken* as to whether one did or suffered something; whereas it makes no sense to talk of being *mistaken* as to whether one is now in pain: that my points (1) and (2) cannot be got round by as it were appealing to a possible incorporeal being himself (or itself) to settle decisively whether or not he (or it) is Myers.

(4) An entirely different sort of point. If Professor Price's speculations are as a matter of fact near the mark: should we not expect that 'communications from spirits' which had had 'in the body' the richest image life would relatively be both (probably) more abundant and (certainly) indicative of a more vivid life 'in the next world'? This might turn out to be a useful research pointer: for 'It is in the field of imagery that some of the most extreme human individual differences are to be found' (*A Textbook of Human Psychology* by Peter McKellar, p. 177).

ANTONY FLEW

King's College, Aberdeen.

COSMOLOGY AND PSYCHICAL RESEARCH

SIR,—It has frequently been suggested that telepathic communication is instantaneous and is in no way impaired by or dependent upon the distance between agent and percipient. What evidence is there that this is true? I know of no experiment that has been or can be devised, having regard to the fact that we inhabit this planet, whereby such a hypothesis can be sustained. There is no evidence (if survival is true) that such concepts as space and time, as we understand them, prevail in the 'spirit world'. There is no evidence (if survival is true) that the 'spirit world' is or remains contiguous to a solitary planet that is alone inhabited and is travelling, relative to its solar system, relative to the galactic system of which it is a member and relative to extragalactic nebulae, at a velocity that is incomputable. There is little doubt that there are vast numbers of planetary systems in the universe, not dissimilar from our own, many of which, if they exist, are so placed relative to their respective suns, by reason of the fact that they are planetary systems, that sentient life, in the opinion of many cosmologists and others, may well exist upon them. If the greatest velocity attainable in the universe is that of light and the velocity of light is constant to all observers thereof, irrespective of their motions (unlike the velocity of sound) relative to one another, is telepathic communication (if survival is true) within the 'spirit world' and between the 'spirit world' and ourselves, amenable thereto? What do we understand by ubiquity in relation to telepathy, precognition and kindred phenomena? While matter, as we understand matter, cannot exist without space, it is now recognized that space cannot exist without matter, in the sense that space is conditioned by the nature and presence of matter, in a manner which is little understood. For these and other reasons, diacosmic simultaneity is incomputable.

Research in the realms of metaphysics, no less than scientific research of a more orthodox category, must conform with and rely for its validity upon recognized standards of proof. No telepathic or spiritualistic hypothesis can continue to ignore the conclusions, tentative although they may be, of science. I shall be most interested to learn the views of others upon these problems.

DENIS CHESTERS

Brighton, Sussex.

NON-CAUSAL SYNCHRONICITY

SIR,—I am much interested that the expression 'Non-causal Synchronicity' has been coined. Whether a fact or not, does not

such an idea underlie most superstitions? No one supposes that thirteen at table *causes* a death, but the notions hang together, and can be dismissed together: and there is a *feeling* of connexion.

Now that physical science admits spontaneous, i.e. non-causal, movement in atomic or sub-atomic action, may we not also look for non-causal action elsewhere?—inconceivable as it is!

AGNES FRY

Brent Knoll, Somerset.

'TELEPATHY AND SPIRITUALISM'

SIR,—Dr West has adequately replied to Dr Hettinger's arguments in the January–February *Journal*, but there is one point in Dr Hettinger's letter which refers directly to my conduct of an experiment and which, I feel, calls for my comment. Dr Hettinger writes: 'Let me point out a fact not mentioned by Scott in his review. Scott changed to the so-called "control" pictures *after* he had received my report of the sensitive's impressions.' Both my memory (which is admittedly most unreliable so far back) and the published report (which is not quite explicit) suggest to me that this is not correct, and I am tempted to ask Dr Hettinger how he is able to state it as a 'fact'. But the important point is that it is wholly irrelevant. If I had wanted, I could have 'cooked' this experiment in innumerable ways. First, since I was myself the agent I could simply have refrained from concentrating on the pictures to be transmitted. No amount of witnessing or precautions taken by others could have prevented this. Indeed, it is a perfectly plausible hypothesis that my unconscious actually did something of this sort in response to a (supposed) desire for a negative result. This would not of course explain the *positive* results obtained on the control pictures and Dr Hettinger's comment that I was apparently a very good subject. Conscious fraud would have been necessary to achieve this (incidentally unforeseen) result. But I could easily have managed this whether I had 'changed to the controls' before or after receiving the report of the sensitive's impressions. Personally I feel that Dr Hettinger is perfectly entitled to suspect me of cheating. What he is not entitled to do is to carry on as though nothing had happened, as though he were under no obligation to answer my arguments or to repeat my experiment with someone of better reputation, or greater impartiality, than myself.

My experiment has been taken too seriously. It is not a conclusive proof of anything. It was simply the last—and perhaps most dramatic—of a series of arguments tending to show that

MAR. 1953]

Correspondence

there is nowhere in Hettinger's work adequate evidence of psi. All I maintain is that these arguments are easily strong enough to demand either an adequate reply or an improvement in Dr Hettinger's methods in the future.

CHRISTOPHER SCOTT

London, W. 14.

